A. M. D. G.

BULLETIN

of the

American Association of Jesuit Scientists

(Eastern Section)

For Private Circulation

CONTENTS

SCIENCE AND PHILOSOPHY

A RECORDING PSYCHOGALVANOMETER

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The present apparatus developed in the psychological laboratory at Fordham University resulted from our efforts to devise an instrument for the exact recording of human emotional variations. There is a long history of investigation of the nature and characteristics of the psychogalvanometric reflex. Instruments for these investigations ranged all the way from the simplest type of galvanometer to the Einthoven string galvanometer. From the time of the early Greek physicians, it was known that emotional changes even of a minor character could be detected by changes in heart rate, pulse and respiratory changes. These methods were refined in more recent times by Marston, Larson and Keeler. In our early investigations we employed pressure and respiratory apparatus to discover that they were not adequate to record many important emotional changes which not only were introspectively and consistently reported by subjects, but were definitely recorded by electrical instrumentation.

Our work began with an ordinary Wheatstone arrangement with a sensitive galvanometer which had a light and scale attachment. The ordinary laboratory D'Arsonval galvanometer proved unsatisfactory because of its inability to pick up small electric variations. We discovered that the more sensitive the galvanometer, the more sources of error crept in to our results. The greater sensitivity involved a longer periodicity in which the primary results we were endeavoring to record were masked by secondary and tertiary electrical phenomena. The next step in our investigation was the development of a cathode ray oscillograph with photographic recording attachment hooked up with a direct current amplifier. We succeeded in developing a fairly good amplifying system, the solution of which took considerable time. An ordinary cardiograph deals with a 30 cycle phenomenon. We had to devise an amplification system that

would respond immediately and consistently to a phenomenon which was producing one variation every four to ten seconds. For this purpose we found that alternating current amplification was altogether unsuitable and consequently we were obliged to develop direct current amplification. The whole apparatus is now part of our museum. It became increasingly evident, that to utilize a photographic record made by the movements of the beam of light in the cathode ray oscillograph, lengthy protocols were necessary for exceedingly small units of every experimental process. We decided to change the apparatus and to develop a direct and visibly recording device which would eliminate a great deal of the details required in the recording of experimental data when we employed the cathode ray oscillograph.

FIG. 1
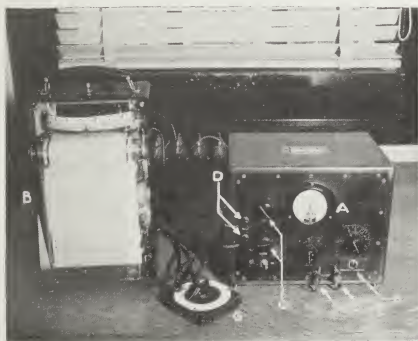
The completed apparatus is shown in Figure I. The subject is connected by means of two german silver contacts placed in the palm of each hand, the leads from which are led into the binding posts, D. The box to the right of the picture contains the amplifying and rectifying systems. After amplification the current is drawn out through binding posts, E, carried through the shunt G, and thence to the recording milliammeter, B. There is an input circuit which enters through the rear of the box. The subject is balanced electrically by means of the dials, C, the lower of which governs coarse and the upper adjusts for fine readings. The milliammeter, A, which is in series with the recording milliammeter, B, is employed as a check

meter and also as a safety for the protection of the recording milliammeter, B. The switch, H, enables the operator to place the subject in circuit with the recording milliammeter or with a series of resistances for the measurement of body resistance. The dial directly above switch, H, indicates the subject's resistance when a balance has been established. The fundamental electrical concept employed in the principal circuit is that of two balanced electric circuits, the box containing a very complex circuit and the subject the second and more simple circuit. Any disturbance in the electrical balance is indicated by the check meter, A, and is recorded on milliammeter, B. The milliammeter chart is clock-driven and can be varied according to the requirements of the experiment. In work on the emotions we generally employ a chart speed of three-quarter inch per minute.

Amplification was one of the vexing problems. We had several types of amplification and finally settled on a system of rectification and amplification which would be applicable for the majority of cases with which we had to deal. As employed in the present arrangement, the amplification is two-stage and ranges from 0 to 350. This enables us to obtain maximal scale deflections for a subject whose resistance is 100,000 ohms. With resistances less than this amount, the deflections are kept on the graph, which is 4½" wide, by means of the shunt, G. The middle dial directly under the check meter, A, affords another means of increasing the sensitivity of the instrument or of controlling the deflections of the recording galvanometer.

The instrument, as shown in Figure I, has been employed for a little more than a year. When we were satisfied with the sensitivity of the instrument, we proceeded to investigate instrumental errors due to current fluctuation, lag of the recording needle due to friction, hysteresis and heating effects. We finally placed all resistance units outside of the box represented on the right so that the amplification unit would not be interfered with by heating effects. The amplification unit as we employ it at present, is a screened unit.

The first work we completed with the apparatus was an investigation of the differentiation between emotion and sentiment. The results of this experiment were read at a meeting of New York Branch of the American Psychological Association, held at Fordham University last April. We were able to show that there was no statistical significance between intensity of sentiment and degree of physiological concomitance. There is no relation between intensity of sentiment and intensity of the sensory accompaniment. This study enabled us to present a new theory of affective reactions. We inferred from our study that feeling, or that which we interpret as pleasure or unpleasure, is a basic activity of the affective order. Emotions and sentiments are specific instances of feeling. Emotional reaction occurs when there is feeling with a predominant sensory factor. Sentiment is feeling with a predominant intellectual factor. In sentiment feeling is not necessarily accompanied by the same

amount of organic or physiological changes which are present in emotional reaction.

We had employed the instrument to detect emotional changes which accompanied deception. Considerable discussion was brought to bear on the applicability of an instrument of this type in actual criminal circumstances. This summer we decided to investigate the reliability of the instrument in criminal situations. We planned, however, to develop a laboratory situation which would furnish a very close approximation to a criminal situation. We believed that a procedure of this type was absolutely necessary in order to establish the reliability of the instrument before it should be applied in any detailed investigation of criminal activity. In the experiment we employed 50 groups of college and graduate students, male and female. Each group was divided by drawing lots into two sub-groups. The first sub-group in each test was presented with a closed box which contained a valuable article, a twenty dollar bill, an expensive watch, jewelry or perfume. The instructions given this group were the following: I would like you to consider that you three have conspired to steal this box. When I leave the room, open the box and you will discover a valuable article. This article cannot be divided among you three. So, draw lots. And the winner of the draw will take the article enclosed in this box. Make this draw after I have left the room. Subsequently, I shall ask you questions. All members of this group were instructed to deny personal guilt, any knowledge of the guilty person and to deny possession of the article in question. The responses to all other questions were to be truthful. A final condition was placed: If the person who won the draw and so possessed the article in question succeeded in deceiving the experimenter, he or she could keep the article. If the experimenter discovered the guilty person, this person would be obliged to perform a penalty to be named by the experimenter after the whole group had been tested. The second sub-group in each test was unaware both of the guilty person and of the article taken by the guilty person. Hence in each test our subjects fall into three groups:

a. the person who took the money or jewelry or the watch or other article;

b. the person who knew both what was taken and who the guilty person actually was;

c. the controls, those who knew neither what was taken nor who the guilty person actually was.

The results of the experiment were rather satisfactory. Forty-nine of the fifty guilty persons were detected by our procedure. Of these forty-nine, nine were discovered on a re-examination, but in each of these nine cases there was definite evidence of either com-

plicity or guilt on the first test. In the accomplice group of eighty six, sixty-four or seventy-four per cent were definitely established on the first test to be accomplices and not guilty persons. In all but two of the remaining twenty-four, the fact of complicity was established by second tests.

There were ninety-one controls utilized in the fifty group tests, eighty-two of this number were established on first tests to be inno cent. Startle effects and the limited number of questions we employed most probably interfered with better results on first tests, both here and in the complicity group. The ninety per cent efficiency for the control group was increased to one hundred per cent by re examination.

Seventy-five per cent of the subjects employed in this study were used in four or more group tests. Our object in utilizing these subjects so often was to test the value of our technique in the con ditions of possible diminished emotional response due to the fact that the subjects might become familiar with the procedure. In some of these cases, the responses to the critical questions showed a diminished reaction, but only where all reactions of the subject to critical and non-critical questions were proportionately diminished.

During the progress of the experiment we decided to check our results against those furnished by a Keeler Polygraph. We wished to contrast the relative reliability of our instrument and the poly graph. We utilized twelve groups which involved sixty-two persons, each one of whom was hooked up to both instruments during the process of examination. The comparison of our instrument and that of the polygraph revealed the following:

A. Where we had 100% correct in the detection of guilt, the polygraph established 54% doubtful and 46% negative.

B. In the accomplice group we had 85% correct on first tests where the polygraph had 92% which were either negative or doubtful.

C. In the control group we had 95% correct on first tests where the polygraph had 47% correct.

We do not wish to put any final value on these differences. Before presenting a final conclusion we should like to spread the study over a wider range of cases. The results of this study were presented before the September meeting of the American Psychological Association at Hanover.
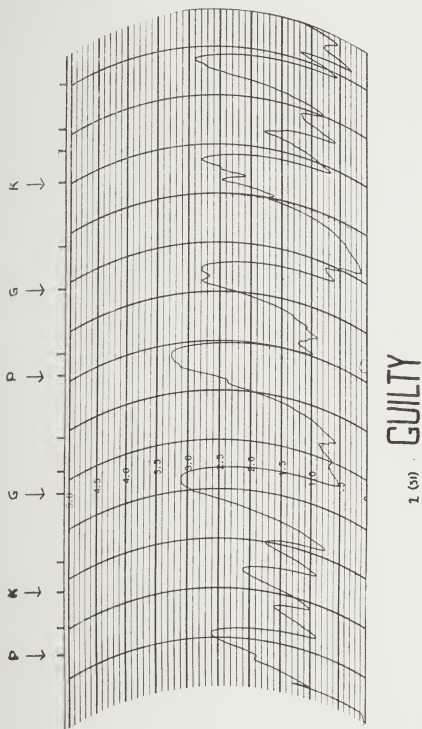
FIG. II

Figure II is a record of a guilty person in this experiment. The short vertical lines on the top of the graph indicate the times at which questions were asked. The vertical marks which are lettered K, G, and P are critical questions. The graph reads from right to left. Other questions are non-critical, that is, have no bearing on the mat-

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ter for which the subject is being particularly examined. At K the subject was asked if he knew who took the money. At G the subject was asked if he took the money. At P the subject was asked if he had the money on his person or had it put away in some other place. There are many interesting features of emotional reactions portrayed in this graph. Space will permit the indication of merely a few. This subject had a great deal of emotional disturbance responding to the first question. The second and third questions show diminished emotional reactions. Note the contrast of the deflections coincident with the answers to K, G and P with the deflections coincident with the responses to the non-significant questions. The subject in this case denied knowledge and guilt and possession. The record clearly shows that he was lying in his responses to all three. The last four deflections indicate an increased resistance on the part of the subject which is an additional significant factor. It manifests very generally the presence of a release from the emotional strain which indication makes the preceding readings extremely significant.

This instrument has been employed in several instances of actual crime detection with invariable success. The records of these cases would need a special article. But it is important to note that in the investigation of emotional reactions, the sensitivity of the instrument is only the starting point for accurate investigation. We constantly employ in this work all the experience that has mounted up in the course of the last four years. The graph in Figure II presents an almost classic reaction type. But not all records are as clear-cut as this one. There are many variations in the initial, in the middle and in the final phases of emotional reaction. As time permits we plan to employ the technique we have developed in the investigation of emotional reaction types with the object of discovering whether or not there is a consistent incidence of emotional reaction referable to physical and physiological typology. There are several other problems being contemplated, the chief of which are the value of an instrument of this type as an objective control of introspection and a study of its possibilities in the testing of candidates for various positions where emotional control and ability to change judgments in complex emotional situations are necessary. We plan also to continue some work already begun on the discrimination of feigned from real delusions.

THE PHYSICAL LAWS AND PROBABILITY*

RAYMOND J. FUSSNER, S.J.

Till within recent years science was content to accept the authority of philosophy for the validity of its fundamental principles, and scientific endeavor occupied itself with extending the application of these principles to an ever widening domain. The picture today is changed. The physicist first became suspicious, and when his observational experiments failed to verify his philosophic beliefs, he began to shift his structure of science onto a new foundation. This mutation however has not been confined to physics alone. *Science* (Feb. 28, 1936) gives a report of Professor Niels Bohr, explaining in a lecture the necessity of extending the famous uncertainty principle to the sciences of biology and psychology as well. This change in the foundation has naturally necessitated a remodeling of the superstructure, so that advanced physicists are now concerned with applying their new principles to the findings and data of the past. Professor Eddington is doing his part in this work of reconstruction—fitting the concept of the physical laws onto its new corner-stone of Probability.

Before taking up a closer study of Mr. Eddington, it would help to have some notion of the hope of the present-day physics. Properly speaking it is not physics at all—the science of sensible nature and of the real causes concerned therein—but rather the mathematics of sensible phenomena. As Maritain well expresses it in one of the conversations of *Theonas* the sage, "it is an intermediate science, a science which is formally mathematical, of which physical data merely furnish the *matter*."(1) And though physics, as Mr. Eddington informs us, aims at explaining or formulating the physical laws, or the laws which govern the regular recurrences of experience, the matter with which physics deals, is not the actual objective universe of experience, but the submicroscopic units, the atoms and electrons, or quanta of which the material universe is ultimately composed. Thus for example, to the modern physicist, a desk or chair is nothing more than a host of tiny electric charges like a swarm of gnats, darting hither and thither with inconceivable velocity. The physicist attempts to formulate the laws for these individual quanta, and finding their contain a character of probability, he does not hesitate to transfer this quality to the ordinary physical laws, which he assumes, are merely a further adaptation of the quantum laws, i.e., laws which in themselves are nothing more than statistical averages that obtain when the number of individual quanta is very great. And so we have the thought-provoking statement of Mr. Eddington—physical laws are obeyed because it is too improbable that they should be broken.

(1) Maritain, Jacques, *Theonas*, p. 67.

In concluding to the element of probability in these quantum law the physicist was influenced by two main considerations, the Heisenberg Principle, and the notion of Indeterminism. Let us first take up a consideration of the latter. The most ordinary meaning of Indeterminism is freedom of choice, and usually is referred only to man. Does Mr. Eddington apply indeterminism to inanimate nature in the same way it is applied to man? In answering this question, we can consider one of the instances he cites, that of a radioactive potassium atom. A peculiar characteristic of this atom is that there occur at irregular intervals, little outbursts or explosions. He readily admits and states that scientists have information which shows that the burst of the potassium atom is *not* undetermined, i.e. that it *is* the effect of an antecedent cause. But coming now to the *actual time* of the outburst, the situation is not the same. He describes it thus:

> Nothing is known as to the time when a radioactive potassium atom will burst except that it will probably be within the next billion years. If, however, we observe that it bursts at a time 't' we can ascribe to the atom the *retrospective* character K_t, meaning that it had all along the property that it was going to burst at time 't'. Now according to modern physics the character K_t is not manifested in any way—is not even represented in our mathematical description of the atom—until the time 't' when the burst occurs and the character K_t having finished its job disappears. In these circumstances K_t is not a predetermining cause. Our retrospective label adds nothing to the plain observational fact that the burst occurred without warning at the time 't'; it is merely a device for ringing a change on the tenses (2).

What does he mean? Simply this,—IN PHYSICS, the character K_t cannot be considered as an effect, since empirically there is no way of knowing anything about its cause. The philosopher with his notion of the *ratio sufficiens* will insist that it is an effect. Eddington does not deny that. But for a physicist who limits himself merely to experimental knowledge and deals with the *universe of physics*, this K_t character is simply an event unpredictable from a cause, since empirically that cause does not exist. That is the motion behind some of the author's statements, which in themselves seem almost meaningless, e.g. "Modern physics does not deny the principle of causality", it merely states that "Determinism and the Principle of Causality are not the basis of physics as conceived today." (3)

Let us return again to this Indeterminate K_t character and view it from another aspect. As we gather from a later discussion, this indeterminateness implies also that this K_t character cannot be explained in a purely mechanical fashion, i.e. merely in terms of extrinsic pushes and pulls. Thus, I may know all the extrinsic forces,

(2) Eddington, Sir Arthur, *New Pathways In Science*, p. 96.
(3) *ibid.*, p. 74.

the position of the electrons within, and all other information which science can supply, and yet I will not be able to tell the exact time at which the burst will occur. Why not? Because there is still another factor which has not been considered. There is some intrinsic force, for which a mechanical or mathematical law cannot be written. In other words, nature is not a machine. In order to make this notion a bit clearer, let us draw an analogy to a higher level of being. My analogy is to be with what I think is a parallel in the case of a dog. Come now Roger, demonstrate this for the men. Roger is walking up the path towards the front gate. On the street, in neither direction is there a cat, a person, a pleasant odor, in fact nothing at all which would attract him. He reaches the front gate; which way will he turn—to the right or to the left? Really there is no way of knowing until after he has turned, i.e. only after the event has taken place. Empirically, the event cannot be considered as an effect of some cause, and keeping on the same level of experience, we might call it *indeterminate*. Does this mean that we are attributing the freedom of man to the dog? I think not. In fact, in Scholastic philosophy we have a name for just such an event, we call it *freedom of spontaneity*. Now let us descend the ladder of nature which we have mounted for the purpose of this analogy. Consider this perfection of spontaneity decreasing proportionately the farther we go down the ladder, and coming finally to our atom or an electron, we find the analogous character, what we might call a bit of lee-way in responding to extrinsic mechanical force.

Thus there is an intrinsic determination, which older physicists did not take into account. For the physicist, it is something new which his research and speculation have uncovered, and is certainly unexplainable by any notion of purely mechanical causality. Since that is the only kind of causality of which the physicist has any knowledge, he logically says that his science is not based on such a principle, which is known to science under the name of Determinism. We most readily admit that nature is not just a machine, wound up, and forging blindly ahead. It has an intrinsic character, which in Scholastic philosophy we call the Law of Nature or the inclination found in nature, or in a wider sense, the substantial form. To this Eddington in one place refers as the *energy of constitution*. Since it is not the part of the physicist to study the nature, he takes the electron as he finds it, having this property of being allowed a bit of lee-way, and as such he does not consider this characteristic as an effect, produced by the inner nature as its cause. Consequently, an empirical science built upon the activity of such an electron can truly be said not to rest upon what the physicist calls the principle of causality or Determinism.

To get a still broader notion of the idea of Indeterminism, let us look at an electron with the principle of Heisenberg limiting our

scientific method. This principle, as I mentioned before, was the second factor leading physicists to their notion of probability. Suppose I am trying to collect data about an electron. I send a photon of light to the electron, and it reports back that the electron is in some definite position. But let us not forget that according to Planck's Quantum Laws, this photon of light contained energy, not much it is true, but still enough, that when it fell upon the electron and thus reported its position, the electron received a slight kick and consequently moved, very slightly perhaps, from the position in which it was reported to be. If I send another photon to discover this new position, the same result is repeated, so that if the observer is persistent, he becomes like the comedian with an armful of parcels, each time he picks up one, he drops another. Or take another condition,— the electron is travelling at a very high speed. If I slow it down enough to determine its exact position, at the same time I cannot know its exact velocity; if I want to know its exact velocity, then at the same time I cannot know its exact position. And what is the conclusion? In this universe of physics, everything is known with only a partial knowledge. To accommodate just such knowledge, the concept of fog was introduced, whereby the position and velocity could be averaged out, and the probable character of each expressed in a single formula. But by some strange alchemy, known only to physicists, this fog, meant at first to represent only an incomplete knowledge, or a subjective state of the mind, has been made into something objective. In fact, it has become the actual stuff of the universe as it is conceived in physics, but in order to give it a more dignified name, it has been rechristened " ψ " or more often probability. Therefore states Mr. Eddington:

> We ought to say that on the present view the content of the universe consists, not of particles but of waves of " ψ ". But at the same time it must be realized that a universe composed of " ψ " waves necessarily contains a large subjective element. Its constituents collect into drops or dissolve into fog according as our knowledge of them happens to be precise or partial. It is a stage whereon the spirit-actors materialize and dissolve as we turn our attention one way and another.(4)

Since then, science has at its disposal only partial knowledge (Heisenberg Principle) and deals with probability as its very foundation, it is not surprising that the laws too should share in its probability.

Thus we see into what a queer mental tangle the science of physics has involved itself. It has only one aim, to discover laws which fit its mathematical formulae, but of the actual things in the universe, which these laws are supposed to govern, the physicist pretends to know nothing. But in so doing, as Maritain well observes:

If one seeks the laws without being able to know the *causes*

(4) *ibid.*, p. 45.

in themselves, one is exposed to the risk of taking mere fictions for causes or for real beings—and this must happen whenever, in spite of the firmest resolutions, one yields to the intellect's instinctive attraction to *being* and so proceeds to attach some physical and ontological value to those *entia rationis* which have been constructed as a support for the language of mathematics.(5)

To return again to the Heisenberg principle—this probability or indeterminacy of knowledge has now, as we have observed, been projected into nature itself, and instead of the *knowledge* of nature being indeterminate or probable, and nature remaining determined, the process has been reversed, and nature becomes indeterminate, while our knowledge of it becomes very determined or certain. That indeterminism or probability applies only to nature and has nothing to do with knowledge, or the subjective state of the mind, Mr. Eddington is at pains to make clear. Thus he advises us not to become impatient with nature, as though she were trying to prevent us from seeing too far into the future, since the future is *not predetermined* and nature has no need to protect herself from giving away plans which she has not yet made. He also demonstrates his point of view by an example: Heat flows from a hot body to a cold body, because it is too improbable that it should flow the other way. Does this mean that if I go on long enough putting kettles on the fire, some day I may expect to find a lump of ice in the kettle? By no means. For to quote Mr. Eddington:

> My whole reason for accepting the laws of nature rests on the assumption that improbable coincidences do not happen in my experience. The ice incident, for example, will never happen to *me*. So confident am I, that even if tomorrow I find ice instead of boiling water in the kettle, I shall not explain it as the exception implied in the statement *too improbable*. Probably I shall exclaim 'The devil's in it'. That indeed would be the more rational explanation.(6)

Then too, when the term improbable is applied to the laws of nature, a new relation, as Eddington tells us, arises between what we commonly term the improbable and the impossible, so that while we do experience coincidences which are described as extremely improbable, such improbability is of an entirely different order from that concerned in a discussion of the physical laws.

Obviously, it was with no question in mind of the validity of the principle of induction that Mr. Eddington asserts that physical laws are obeyed, not because it is too impossible, as older physicists asserted, but because it is too improbable that they should be broken. This change in the formulation of its laws science deemed necessary in order to bring them into harmony with its newly discovered fundamental principles. But has this shift onto a new foundation been justified?

(5) Maritain, *op. cit.*, p. 68.
(6) Eddington, *op. cit.*, p. 62.

Scholastic philosophy *had* furnished a firm basis for science and in particular for the laws of nature, in the principle of causality or sufficient reason. We subject the data of experience to a critical examination, and find, by means of the principle of sufficient reason, the ultimate cause for the regularities of experience in the unchangeable nature or essence of the object. It is because this nature is itself unchangeable that we have certainty that events will occur in the future as they were in the past, prescinding of course from miracles. But this method is valid, and here is the important point—only if I rise above the level of experience to the higher level of reason. If I refuse to rise above experience—the principle is useless.

The attempt to confine himself merely to experience is perhaps the biggest failing of Eddington and for that matter of most present day scientists. I have called it an attempt, for Empiricism is merely that, an attempt, but not a successful one. Every scientist is first and foremost a man. He has reason, an intellect, and consciously or not, willingly or not—when once an experience is had, this reason with cold hard logic formulates its conclusions. The scientist cannot deny it; he may attempt to abstract from these conclusions or disregard them, as Mr. Eddington continually reminds us that he is doing, but never succeeds. To take just a few instances—in science, equilateral triangles have equal angles because that is what actual measurement has confirmed of them; we may not rely on the mental analysis of the idea *equilateral*. Or again, in dealing with the problem of *time* we may not accept the data of consciousness. To quote Mr. Eddington:

> As scientists we are anxious to make the scheme of the physical universe as self-contained as possible. We do not want to be dependent on consciousness, which is outside the scope of physics, for so fundamental a physical distinction as that between past and future.(7)

The principle of causality or sufficient reason does not register on a galvanometer nor leave its tracings on a photographic plate and science will have nothing to do with it.

And why this attitude of extreme Empiricism? Because science has behind it a brilliant record of accomplishments, the triumph over earth and water and air, all of which it thinks it has achieved merely by keeping to data of experience, and not rising above it. I think Mr. Eddington is a good example of an Empiricist who simply could not hold out against reason. His whole idea of Indeterminism is proof against it. He has tried to narrow himself too much, instead of allowing reason to take its course, and lead him to truth. If men would only follow reason instead of attempting to lead it, nature which now seems to them so inexplicable, since they have shrouded her over with the dark mantle of their own prejudices and presuppositions, would of herself open and reveal the beauty of design and teleology, which the Divine Artificer has concealed within her.

(7) *ibid.*, p. 52.

BIOLOGY

A CASE OF ANTEVERSION OF THE HEART OF THE FROG

Rev. Paul L. Carroll, S.J.

Structural anomalies are not so commonly found in the frog. When one considers that thousands of frogs are dissected each year in the zoology laboratory, that they are used for physiological and pharmacological experiments and that investigators the world over consume them, it is indeed surprising that instances of the displacement of internal organs are so seldom met, or if found not reported.

A case of anterversion of the heart of the frog recently came to our attention while doing some extensive work on the vascular system. The case in question is hardly of any great scientific value, but of some interest to those working continuously with frogs, and perhaps, helpful in the study of the normal development of the heart.

The frog was a healthy male 'leopard variety' weighing about 45 grams. It had been obtained from Oshkosh, Wisconsin early in December. The abdomen was opened by an incision along the linea alba from the symphysis pubis to the xiphisternum; the whole pectoral girdle was slightly elevated so that it could be cut without injury to the pericardium. The fibers of the M. obliquus abdominis internus inserted into the pericardium were liberated. The several portions of the sternal bones were gradually separated in order to get at the pericardium which was slit along the mid-ventral line to expose the heart.

The heart of the frog is so familiar to all that a very few words of description will suffice. The thick-walled conical ventricle points posteriorly and slightly backwards; the truncus arteriosus lies on the ventral surface of the right auricle and extends from the auricular-ventricular groove anteriorly and towards the left where it bifurcates into two main branches. The larger right auricle is separated from the smaller left auricle by the interauricular septum. A ventral view of the heart in its normal position is shown in figure 2; a frontal section of the same heart is seen in figure 1; the magnification in all figures is 2x. For the drawing of the normal heart, a healthy male about the same size and weight as the frog with the abnormal heart was chosen.

For convenience let us pass a median and a transverse plane through the normal heart. The median plane would bisect the apex of the ventricle, cross the auricular-ventricular groove, run between and parallel to the truncus arteriosus and the interauricular septum, and sever the left branch of the truncus arteriosus. Let the transverse plane run about midway between the most anterior border of the right auricle and the tip of the ventricle. It would just about
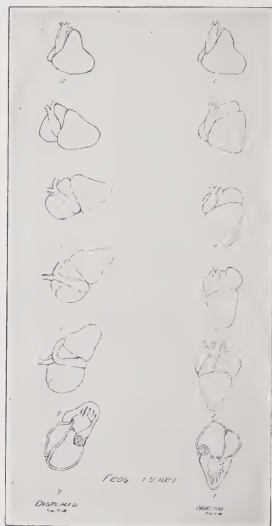
coincide with the auricular-ventricular groove. The intersection of the axes of these planes would indicate in a general way the center of the heart. The sinus venosus on the dorsal surface of the heart would be bisected by the median plane, while the transverse plane would fall slightly posterior to the right and left precavals.

Now if the normal heart is twisted at the imaginary intersection of the two planes and turned to the left at about an angle of 170 degrees, a somewhat modified type of sinistrocardia, or anteversion as we have called it, will be obtained. Figure 8 is an exact

drawing of the misplaced or anteverted heart; figure 7 shows a frontal section of the same heart.

Let us make some comparisons between this misplaced heart in figure 8 and the normal heart seen in figure 2. The sinistroversion is best understood by visualizing the same median and transverse planes used for the study of the normal heart. In the misplaced heart the ventricle is normal in shape and size; its position, however, has been altered; the auricles, too, are normal; the truncus arteriosus is quite elongated along the base of the right auricle, extends over the interauricular septum and comes to rest on the left auricle; the left branch of the truncus arteriosus is enlarged and curves downward over the left auricle and disappears behind the ventricle at the auricular-ventricular groove. An examination of the dorsal surface of the heart did not reveal even a partial transposition of the sinus venosus. The action of the living abnormal heart was carefully observed and compared with the beating of a normal heart until we were satisfied that there was no evidence of any functional disturbance. Moreover, cinnabar was injected into the abnormal heart with the result that an average arterial flow was obtained. Finally, a frontal section of the heart was made which shows in figure 7 that the valves and internal structures are all normal.

The question of the origin of the misplaced heart should receive some consideration and the following possible explanation is offered. When the heart of the tadpole has reached the stage where the three chambers are quite distinct and well developed, the heart hangs in a vertical position somewhat as in figure 6. The ventricle gradually falls or is pulled posteriorly during later development which we have represented by the stages in figures 5, 4, 3. Finally the usual position as seen in figure 2 is reached and maintained. If we start at a relatively similar stage in the development of the misplaced heart we would get a heart represented by figure 12 which does not differ from figure 6. Thus we assume that the anteverted heart grew in a normal way up to the time it takes this position. From this stage on we assume that the heart turned anteriorly instead of posteriorly. Figures 11, 10, 9 show hypothetical stages in the process of distortion leading to the condition found. One might ask what influence the reabsorption of the coiled intestine has on the position to be taken by the heart; or the effect of the protrusion of the fore and hind legs; or the force of the elongation of the abdominal muscles. Even if we cannot give a final and completely satisfactory explantion of the position in the pericardial cavity taken by the misplaced heart, still it is interesting to have seen such a case of anteversion.

—Biology Department, Marquette University, Milwaukee.

PARAMECIUM MULTIMICRONUCLEATA Vs.
PARAMECIUM MULTIMICRONUCLEATUM

Rev. John A. Frisch, S.J.

In 1910 Powers and Mitchel[1] reported and described a new species of *Paramecium*. Powers speaks of it as a multimicronucleate type; Mitchel calls it *Paramecium multimicronucleata*. Article 14 of the International Rules of Zoological Nomenclature, speaking of specific names, states that adjectives, used as specific names, must agree grammatically with the generic name. *Multimicronucleatum* is used by Mitchel as a descriptive adjective, and since *Paramecium* is a neuter singular noun, the descriptice adjective must also have the neuter singular form.

Landis,[2] Wenrich,[3] Lieberman,[4] King,[5] Giese,[6] Diller,[7] Duodorff,[8] have followed Powers and Mitchel in using *multimicronucleata*. But Hance,[9] Lucas,[10] Sranghöner,[11] Müller,[12] Glaser,[13] Köster,[14] Frisch,[15] Jones[16] and Oliphant[17] use the grammatically correct form, *multimicronucleatum*.

Article 19 of the International Rules of Zoological Nomenclature states that "the original orthography of a name is to be preserved unless an error of transcription, a lapsus calami, or a typographical error is evident." An error of transcription and a typographical error can be ruled out. Can the mistake be called a lapsus calami? Probably not. How, then, can the change to the correct form be authorized?

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The above NOTE I had published in *Science*—September 25, 1936; Vol. 84, No. 2178, p. 290.

The only answer I received was from Dr. C. W. Stiles of the Smithsonian Institute, Washington, D. C., Chairman of the International Commission on Nomenclature. It read as follows: "Your article in *Science* for September 25 has been brought to my attention by Professor Fantham of Montreal. There are many cases like this in zoological literature, and it has been customary to correct the endings. This is authorized automatically by the word "must" in Article 14a of the International Code. Incidentally, I

have seen a reprint this morning in which the author uses masculine, feminine, and neuter adjectives for a masculine generic name, and the same author uses a neuter adjective for another generic name in the masculine."

This answer makes it clear that the rules themselves give us the authority to correct grammatical errors in the naming of plants and animals whenever we come across them.

I could have written to Dr. Stiles in the first place, but because grammatical errors are quite common and widely copied, I felt that an official answer in *Science* would benefit others besides myself. For this reason I suggested to Dr. Stiles that he publish his answer to me in *Science*, or at least authorize me to do so. His answer contains additional information on the correct procedure in nomenclature, which may be of interest to others, and hence I quote it in full. "Please feel entirely at liberty to make such use of my statements as you may wish. As a matter of policy, I think it would be better for you to write the article than for me to do so, because your writing it will show that interest in nomenclature is not confined to members of the International Commission, and I have already been accused many times of assuming the rôle of dictator. I try not to be classified with Hitler, Mussolini, and Stalin!

"There is another point of historical interest which you may possibly wish to add to your discussion. On basis of the rule, in Latin grammar, that the names of trees are feminine, some authors have tried to extend this principle to all plants and to use only the feminine in botany; they have also tried to use the masculine for all genera in zoology, thinking thereby to make it easier to distinguish between zoological and botanical genera. This custom was not accepted by the International Commission.

"Another point you might make in your article is that it is only in adjectival names that it is necessary for the specific name to agree with the generic name. Article 14a covers the adjectives. Article 14b covers the substantives in apposition and from the example given, namely Felix leo, one sees that a masculine specific name may be used with a feminine generic name. Thus, suppose there were a species X-us necator and this species were transferred to Y-a, a feminine generic name, then necator would not be changed to necatrix."

A recent book by Schenk and McMasters, "Procedure in Taxonomy", Stanford University Press, Stanford, California, Dec. 1935, besides discussing Systematic Categories, Types, Description of New Species, Specific Names, Synonomy, Storage of Type Material and Latin Terms and Abbreviations, contains a reprint of the International Rules of Zoological Nomenclature with Summaries of the Opinions Rendered to the Present Date. The book would be a valuable addition to the biological laboratory.

NISSL BODIES IN NERVE CELLS

Because of the universal distrust of Cytologic technique which aims to demonstrate and study cell inclusions in fixed preparations, it was early suggested (Mott, 1912, Marinseco, 1912, and Bayliss, 1914) that the Nissl Bodies of Tigroid Masses in nerve cells of Vertebrates, and some Invertebrates, were artifacts resulting from the coagulating action of harsh fixatives which had disturbed the normal cell pattern. It was felt therefore, that in as much as the Ultra-violet photographs of living nerve tissue had disclosed but little, and that of a very vague description (Wieman, 1926), a supravital technique might be devised in which living cells teased out in media would by their own activity ingest suitable dyes, and thereby give a clue as to the condition of these chromophilic inclusions in the normal cell.

At the Zoological Laboratories of Harvard University such a technique was developed. Giant cells from the Cerebellum of young white mice were teased out in saline and subjected to various quantities of a saturated solution of Brilliant Cresyl Blue in absolute alcohol. Satisfactory evidence of the reality of the Nissl granules as large, discrete flakes, or flocculent masses was obtained, together with an absorption pattern of great interest, particularly in the nuclear zone. However, a difficulty arose in attempting to make such demonstrations permanent. Here at the Georgetown University Biological Laboratory an effort is being made to surmount this difficulty, and then to proceed to an investigation of the ontogenetic aspect of these bodies, using the same technique, for it has been claimed that there is a definite relationship between the integrative capacity of a neuron and its content of large or small, discrete granules of Tigroid Substance (Maximow, 1931).

G. W. M. F.

CHEMISTRY

MICRO-VAPORIMETRIC DETERMINATION OF MOLE-CULAR WEIGHTS WITH MACRO-ANALYTICAL BALANCE

Rev. Richard B. Schmitt, S.J.

Each new chemical compound prepared in the research labora-tory must undergo analysis, and its formula obtained by one of the known methods of Molecular Weight determinations. The method taught to students in the beginning of the course in Physical Chem-istry is the A. W. Hofmann (1) and V. Meyer (2) method. This vapor pressure method is usually the first experiment in the Physical Chemistry laboratory. A simple, accurate and rapid method, both for research laboratories and for students, is desirable. The lower-ing of the freezing point, the raising of the boiling point, the con-ductivity methods, need elaborate apparatus and usually skilled operators.

A simple method was devised by Joseph B. Niederl (3) with inexpensive apparatus. This method is a modification of the princi-ple of vapor pressure, as first set forth by Hofmann and Meyer. In this new method, mercury is the sealing liquid, instead of air, used in the Meyer method. This vapor pressure method has proven to be a reliable method for quantitative micro analysis for organic sub-stances.

In this micro method, the usual micro quantities were used, namely 3 to 5 mg. By a series of experiments, we have shown that by enlarging the sample, i. e. from 10 to 20 mg., and using a good **macro** balance, with a sensitivity of 0. 1 mg., accurate results are obtained. Even though a laboratory does not possess a **micro** balance, molecular weight determinations may be made by the vapor pressure method with samples ranging from ten to twenty milligrams.

APPARATUS

Figure 1 shows the simple construction of the apparatus; essentially, merely one flask fused inside of the other, with capillary exit-tube and air-condenser. In the diagram, B is the flask which contains the sample and the mercury; its capacity is 25 cc.
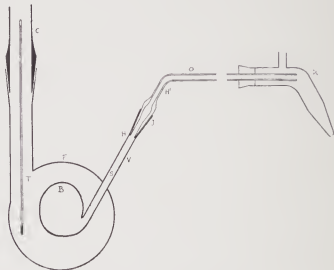
Figure 1.   Apparatus.

O is the capillary exit-tube, firmly held in place by two steel springs at H and J; the outer flask F, holds the boiling liquid; and C is the air-condenser to which is attached a thermometer T. The exact measurements of the apparatus are fully described by Niederl (4). The apparatus may be purchased from Eimer & Amend, Third Ave., at 18th St., New York City, or from the manufacturer, Eck and Krebs, 131 West 24th St., New York City. (Price: eight dollars.)

Mercury and Bath Liquid

The mercury used as the sealing liquid must be free from volatile substances. The following method is satisfactory for purifying the mercury: Prepare a funnel with a very fine opening in the stem. Allow the mercury (reclaimed or C.P.) to fall in a fine stream into a 10% solution of nitric acid, or concentrated sulphuric acid. After separating the mercury from the acid, place the mercury in a large separatory funnel and shake with 10 to 15 cc. acetone. In like manner wash the mercury with distilled water; then place the mercury in an oven and heat to 125° C for one hour.

The bath-liquid should have a boiling point that is approximately 20° C higher than the substance of which the molecular weight is being determined. Water is suitable for all substances that vaporize below 80° C. For higher boiling points, select a convenient liquid, such as p-cymene, citral, benzyl benzoate, etc. The bulb of the ther-

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mometer should dip into the bath-liquid, and may be held in place by
a flexible wire.

PREPARATION OF THE SAMPLE

Liquids. Soft glass capillaries, not less than 2 mm in diameter,
are prepared by drawing out soft glass test tubes in a blast lamp, or
soft glass tubing in a wing-top burner. These capillaries are then
cut 8 or 9 cm in length and cleaned with a soft cloth of chamois. In
a micro burner, allow the walls of the glass tube to come together,
as shown in Figure 2-a. Then draw out the glass, in order to make a
glass rod, which will serve as a handle; as seen in Figure 2-b and c.
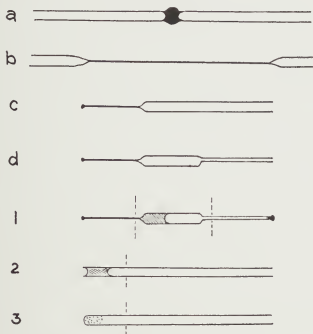
Figure 2   Weighing Capillaries.

Then draw out the open end into a fine capillary, Figure 2-d.
Place a drop or two of the sample on a watch-glass or microscope
slide. Hold the capillary by the handle, gently pass the bulb of the
tube through a micro flame, six or eight times, then immediately
touch the open end of the capillary to the liquid, and as the bulb
cools a portion of the liquid is sucked-up into the tube. Then place
the tube with the sample into a centrifuge, so that the liquid will be
forced to the closed end. Put a small opening in a cork stopper, this
will hold the capillary in an up-right position while it is being
weighed.

Of course, the capillary is weighed before and after the sample
is introduced. The weighing is done on a **macro** balance to 0.1 mg.

The capillary is then cut off at the places marked in Figure 2,
No. 1, and now it can be easily introduced into the mercury flask B,
through the opening S, after the exit-tube O is removed.

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Solids. Capillaries are made, as shown in Figure 2—No. 2 and No. 3. The solid substance to be analyzed may be melted on a micro-scope slide and introduced like a liquid; or the sample may be placed in the capillary by inverting the tube and gently pressing on the solid sample. If it is a fine powder, a small asbestos plug may be used to hold the solid in the capillary. To insure compactness, the capillary is allowed to drop on a hard surface, through a vertical glass tube about a meter in length. Cleaning and weighing of the capillary is done in the usual way.

PROCEDURE

Blank Test. Disconnect the capillary tube O and the air-con-denser C from the flask. Place about 3 cc. acetone in bulb B, and rinse out this inner flask in order to remove organic matter. Put on suction for one minute to remove vapor of acetone. Place the flask in a drying oven for 15 minutes at 110° C to insure dryness. Allow the flask to cool; place an empty capillary in the flask B, then fill com-pletely with mercury to point J. Now attach capillary in the flask B, then fill completely with mercury to point J. Now attach the capillary O, and put on the steel springs. Then fill the capillary with mercury, using a pipette small enough to fit into the capillary tube. Use a thin platinum or copper wire to join the mercury col umn together, and fill tube to the end. Clamp the flask to a ring-stand, and place wire-gauze under the flask. Place a few pieces of porcelain in the flask F in order to prevent bumping and to insure constant temperature of the boiling liquid. Fill flask two-thirds full with the bath-liquid. Attach air-condenser and thermometer Final-ly, attach a suitable receiver for the mercury.

Record the temperature; start heating the liquid, and gradually increase the temperature, so that the mercury will expand uniformly. Continue heating until the liquid boils vigorously for three minutes. Again record the temperature, and weigh the mercury that was ex-pelled on a **macro** balance to $\pm$ 0.01 gram. This weight must be used for correction in the experiment: $C_1 (T_2 - T_1)$.

Allow the apparatus to cool. Remove thermometer, condenser, bath-liquid and finally the mercury and empty capillary. Since the mercury is clean, it may be used again for the actual experiment.

INTRODUCTION OF THE SAMPLE

Clean flask B with acetone, and dry in oven.

Introduce capillary containing weighed sample.

Fill flask B and capillary O with mercury, as described.

Attach to ringstand. Place few pieces of porcelain and bath-liquid in flask F.

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Put air-condenser in place and hang thermometer.

Attach receiver for mercury.

Record temperature T_1.

Heat slowly and then to boiling vigorously for three minutes.

Record temperature T_2.

Weigh the mercury expelled by the vapor pressure of substance.

Calculate the molecular weight.

Calculation

$$M = 62351 \frac{\text{wt} (273.2 + T_2)}{VP}$$

M = molecular weight

wt = weight of sample

T_2 = final temperature

$$V = \frac{g - [c_1 (T_2 - T_1)]}{d} - vs$$

V = volume of vapor

g = weight of displaced mercury

c_1 = correction for expansion per 1° C.

T_1 = initial temperature, ° C.

T_2 = final temperature, ° C.

d = density of mercury at T_2

vs = volume of the sample$\dfrac{\text{(weight)}}{\text{(density)}}$(approximately 1 cu. mm. per mg.)

$$P = p_1 + p_2 - p_3 + p$$

P = pressure of vapor

p_1 = barometer reading

p_2 = vertical distance in mm. between the mercury meniscus in the vaporizer and the orifice of the capillary outlet tube

p_3 = vapor pressure of mercury at T_2

p = capillary depression of mercury in outlet tube (+ 8 mm.); temperature reduction of barometer (— 2 mm. at 15° C., — 4 mm. at 32° C.); density reduction of mercury in stem of vaporimeter for that portion of the stem of vaporimeter which is inside of heating chamber (— 1 mm. for $T_2 = 100°$ C., — 2 mm. for $T_2 = 180°$ C., — 3.5 mm. for $T_2 = 320°$ C.)

The net value of p is usually small (about + 0.5 per cent of P) and within the limits of accuracy and precision of this method, therefore, for practical purposes this correction may be omitted.

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TYPICAL MOLECULAR WEIGHT DETERMINATIONS

Substance—Bath Liquid	Wt. mg.	V.	B.p.	T_1	T_2	P	Mol. Wt fd.	th.
Methyl alcohol—Ethyl alcohol	7.1	6.02	64.5	28	78	852	31	32
Ethyl alcohol—Water	12.1	7.09	78.5	30	100	852	47	46
Bromo-benzene—p-Cymene	20.4	4.29	156.2	28	178	839	155	157
Benzyl alcohol—Citral	8.0	2.70	205	30	226	763	111	108
Thiophenol—Citral	9.1	2.81	170	28	226	762	108	110
n-Butyl sulphide—Citral	8.1	1.56	182	29	226	761	142	146
Thymol—Benzyl benzoate	15.5	7.76	231.8	28	310	740	153	150
Benzoic Acid—Benzyl benzoate	14.6	6.78	249.2	29	310	502	127	122

Landolt-Bornstein, Physikalisch-Chemische Tabellen, Vol. I, p. 67;
Vol. II, p. 1335, Berlin, Julius Springer, 1923.

c_1 0.028 gram mercury.

°C	d	p_a
100	13.352	......
180	13.160	9
270	12.947	123
320	12.829	368

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SEMI MICRO QUALITATIVE ANALYSIS

REV. THOMAS P. BUTLER, S.J.

Semi micro Qualitative Analysis, as devised by Dr. Carl J. Engelder and his associates at the University of Pittsburg, is merely an application of the spot plate and drop-reaction-paper technique to any of the usual systems of macro Qualitative Inorganic Analysis, without the use of a microscope. Both the macro and the semi micro methods of analysis employ the same inorganic chemical reaction, requiring the same study of the theory of solutions, ionic equilibria, solubility products, hydrolysis, etc. The same groupings, separations and confirmatory tests are employed in macro and semi micro analysis. The latter differs from the former only in the laboratory technique in three ways. First, the volumes of test solutions used are limited to

one ml. and the reagents are added from capillary pipettes. Second, confirmatory tests are made with drops of solutions on a black or white spot plate or drop-reaction-paper. Third, filtrations are efficiently dispatched with a small hand centrifuge. It takes but little experience with the semi micro technique to convince one that it not only has all the accuracy of the macro technique, but affords the further advantage of great economy of time and materials.

One of the laboratory slogans of the late Fr. George Coyle was: "You do not have to blow up the house in order to prove that hydrogen burns". Likewise, in Qualitative Analysis, as much analytical knowledge can be acquired by completely precipitating AgCl from one ml. of $AgNO_3$ as by using ten mls. of the same. Using a centrifugal rather than gravitational force for filtrations, a precipitate can be separated from the solution in less than one minute. In comparison to the macro technique, this semi micro technique results in saving 20 to 30 minutes out of a 2 hour laboratory period. Since this time saved can be utilized in trying more confirmatory tests, more chemistry can be learned by our students by using the semi micro technique than by using the macro. Although no attempt is made to make the semi micro Qualitative Analysis semi quantitative, as is commonly done using the macro technique, the same pedagogical effect obtains, since the student soon learns that every capillary drop counts for accuracy and precision.

The apparatus required for the semi micro technique is small and inexpensive, an individual student's set costing less than ten dollars. Fischer Scientific Co. sells a very sturdy micro hand power centrifuge for ten dollars. One for every ten students is sufficient. Schleicher and Schull's drop reaction paper No. 601 is to be preferred for qualitative work. However, even this specially prepared paper contains Fe, Ca, Mg, phosphates and silicates. Blank tests must therefore be made when using the paper in testing for these ions. (Cf. Emich-Schneider's Microchemical Laboratory Manual P. 157).

As stated above, the semi micro technique can be applied to any macro system of Qualitative Inorganic Analysis. Dr. Engelder, however, in his book on semi micro analysis (published by Wiley and Sons, N. Y., 1936) has introduced a few uncommon modes of procedure. At the beginning of the second group, pentavalent arsenic is usually directly precipitated as the sulphide from a 2 N HCl solution, before precipitating the remaining metals from a 0.3 N acid solution. This long odoriferous procedure is avoided by Dr. Engelder by reducing the arsenic to the trivalent state by the addition of a few small crystals of NH_4I. In the third group, Fe, Mn, Cr and Al are first precipitated as hydroxides by conc. NH_4OH, leaving $Ni(NH_3)_6$, $Co(NH_3)_6$, and $Zn(NH_3)_4$ ions in solutions with groups 4 and 5. From this filtrate, Ni, Co and Zn are precipitated as sulphides by H_2S. In the fourth group Ca is separated from Sr and Ba by dis-

solving the evaporated nitrates of Ca in acetone in which $Ba(NO_3)_2$ and $Sr(NO_3)_2$ are insoluble.

As regards the confirmatory tests, many of the micro modifications of the standard macro test have been introduced. In the preliminary work of each group, after giving four or five confirmatory tests for each ion, generally with a remark as to specificity and sensitivity, six or eight additional tests are merely mentioned. A very commendable feature of Dr. Engelder's book is the list of references to the literature for each test. These references are given not only to the original literature, mostly German articles, but also to the A.C.S. Chemical Abstracts. While the majority of these tests, a dozen or more for each ion, employ organic reagents, the usual inorganic tests of macro analysis are given first. The use of organic reagents by sophomore students smacks of the "cook book stuff." But after all, this is no innovation, since some of them, such as dimethylglyoxime and alpha nitroso beta naphthol have been used for years in macro systems of analysis. Most of these organic reagents are mentioned in the 1933 revision of Fr. Coyle's Basic Analysis by Dr. Hynes and Fr. Strohaver. The more common of the metallo-organic rections are explained by Dr. Engelder with aid of graphic formulas, from which the student readily perceives that the reactive part of these reagents is generally either an NH_2 or NOH or OH group.

While much is yet to be learned about the specificity and sensitivity of many of the tests, especially for anions, Dr. Engelder has made a notable contribution to the science of Qualitative Analysis. Working with a group of scholastics, who have just completed a course in Basic Analysis using the semi micro technique, according to Engelder, the writer believes that the semi micro technique is far more fascinating than the macro. Furthermore, it is my humble but firm conviction that in ten years or sooner, this so called semi micro technique will become and be known as *the only technique of MACRO Qualitative Inorganic Analysis!*

APPLICATION OF STATISTICAL METHODS TO ANALYTICAL AND PHYSIOLOGICAL CHEMISTRY

Rev. Francis W. Power, S.J.

PART I.

In Analytical Chemistry we are often asked to state the precision of our results; we may want to know whether or not a modification of the experimental procedure introduces a significant change in this precision; we may have to decide on the rejection of some one

Note: The literature references apply to a bibliography which will be given in another paper on this same subject, which will appear later.

experimental figure which differs largely from others of the same series; we may like to set a minimum number of individual determinations in order to attain a certain predetermined grade of precision; and so on for many other similar questions.

One of the commonest methods of studying the metabolic path of a given substance in an animal is to compare the output and distribution of nitrogen, sulphur, or other urinary constituent after the substance has been fed with quantitative values of the same urinary constituent which were obtained while the animal was fed some uniform normal diet over a certain period of time. In general, some change in the picture will be observed. The question often arises, is the observed change of any real significance, or perhaps may it not be ascribed to those ordinary daily variations in the urinary constituents which always occur even in the most carefully conducted control experiments?

In biochemical experiments involving animal growth where groups of animals are being studied similar questions arise. Is the growth of the animals in the experimental groups significantly different from that of the control animals? How soon can one tell when a significant difference has been obtained? How many animals should be used?

When I started looking up these questions I very soon made the observation that they were being discussed very learnedly and at great length by two groups of people, apparently each one only vaguely aware of the work of the other, and each one treating the subject in quite a different way. In one group we find the physicists, astronomers and artillerymen (1); among the other groups are those interested in educational psychology, actuarial work, bacteriology, genetics, and experimental farming (2). A few bridge over the gap between the two methods of treatment (3); but the chemist will have to go through quite a few books before he can make full use of the powerful tools available to him in the methods of statistical analysis. Naturally there are a great many mathematical treatises on the subject (4) which involve some very heavy mathematics. In the appended bibliography, I note only a few of the books which are available on the general subject; Shewhart gives a more extensive list in his excellent book. The classical contributions of the late Karl Pearson and the technical papers of R. A. Fisher constitute a mine of information, although they are highly mathematical (especially Fisher's); the latter's book is usually recommended as the most useful, even for the purposes of the present paper. In it I am concerned not with the mathematical proofs of the various equations which I shall use, but rather with their practical application and the limitations to which they are subject.

The normal probability curve has the general form

$$y = e^{-x^2}$$

but its shape may be altered by varying either one or both of its possible parameters, e. g., m and n:

$$y = me^{-nx^2}$$

In many treatises on physics one finds it in the form

$$y = \frac{h}{\sqrt{\Pi}} e^{-h^2x^2}$$

where h is the so-called 'modulus of precision' (cf. Mellor, Chapter 9). Most writers dealing with statistical analysis, however, do not use h but rather the reciprocal function sigma, the 'standard deviation', where

$$\sigma = \frac{1}{h\sqrt{2}}$$

and the probability equation is written:

$$y = \frac{1}{\sqrt{2\Pi}\,\sigma} e^{-\frac{x^2}{2\sigma^2}}.$$

I shall first explain how the standard deviation is calculated and then take up some practical mathematical properties of the normal probability curve.

Suppose that a homogeneous sample of finely divided iron ore is analyzed by 20 chemists, whose reports are tabulated and averaged up. The mean iron content is, let us say, 21.68%. Next the differences between this mean and each of the 20 individual determinations is set down without regard to sign and each of these differences is divided by 19 — i. e., one less than the number of individual determinations—and the square root of the quotient is taken as the standard deviation. As an example, I set down an actual series of analyses done by my students on such a sample:

Individual Determinations of percent Fe	Difference from the mean	Difference Squared
21.23%	0.45	.2025
21.23	.45	.2025
21.26	.42	.1764
21.45	.23	.0529
21.57	.11	.0121
21.57	.11	.0121
21.61	.07	.0049
21.63	.05	.0025
21.67	.01	.0001
21.69	.01	.0001

21.72	.04	.0016
21.73	.05	.0025
21.82	.14	.0196
21.84	.16	.0256
21.90	.22	.0484
21.90	.22	.0484
21.91	.23	.0529
21.93	.25	.0625
21.94	.26	.0676
21.96	.28	.0784
Mean 21.68	Sum of Diff. 3.76	Sum of Diff. 1.0736
		Squared

$$\text{Standard deviation} = \sqrt{\frac{\Sigma\, d^2}{N-1}} = \sqrt{\frac{1.0736}{19}} = 0.238\%$$

Not the least of the difficulties encountered in studying treatises on statistics is the lack of uniformity among the authors as regards nomenclature. The standard deviation is very commonly referred to as sigma, especially where very large numbers of measurements are available such as in educational psychology, in which case also N is used in the denominator instead of N-1; and we find the definition:

$$\sigma = \sqrt{\frac{\Sigma\, d^2}{N}}$$

In chemical work, however, the number of observations is usually rather small, in which case the use of N-1 prevents one from under-estimating his measure of dispersion; and while neither form is based on an entirely rigid mathematical proof (Bond p. 50, Mellor p. 523) it will be better to use N-1 rather than N. Fisher emphasizes the possible inaccuracy of the standard deviation derived from a small number of observations by defining it initially as

$$\sigma = \sqrt{\frac{\Sigma\, d^2}{N-1}}$$

but referring to it afterwards as the "standard error" S, which is the best estimate of the true standard deviation which one would obtain if very large samples were available. Since the term standard error is coming to be used in a rather different sense, however, by current statistical writers, I shall adhere to the original term standard deviation, but will designate it by S rather than by σ, in order to indicate that it is in our practise only the best estimate obtainable under the circumstances. That is, I will define the standard deviation as

$$S = \sqrt{\frac{\Sigma\, d^2}{N-1}}$$

For purposes of comparison it will be useful to note down what designations are given this quantity by the authors who would ordinarily be consulted by the chemist. Mellor (p. 525) calls it m, the mean error; Daniels, the mean square error; Ostwald-Luther, the mittler Fehler; many physics books refer to it as the root mean square error.

Referring to the graph of the probability equation

$$y = \frac{1}{\sqrt{2\Pi}\ S}\ e^{-\frac{x^2}{2S^2}}$$

and looking upon it from the standpoint of errors we really have a graph of the value of any particular measurement plotted as abscissa against the frequency with which this measurement occurs in the series, plotted as ordinate. The highest ordinate is in the center, i. e., the largest number of the measurements will be at the value of the arithmetical mean; as we go above and below the mean value the number of measurements falls off until we get to the tails of the curve, representing large variations from the mean, where the number of such measurements is very small. Another illustration of the sort of measurements which follow this type of curve would be for example, the stature of a large group of men. The height of "the average man" would be plotted at the center of the curve, the large values of the ordinates in this region denoting that the largest number of individuals possess about the average stature; the small values of y toward the two tails of the curve representing the small numbers of dwarfs at one end and the small number of giants at the other. Or we may imagine that the curve represents the intellectual ability of a large group of school children; the largest number (high ordinates) possess average intelligence; smaller numbers (medium ordinates) are either bright or dull (medium plus and minus values of x) and very small groups, coming at the two tails of the curve are either morons or geniuses (y small, x large).

The most important point to bear in mind is that the exact mathematical relation

$$y = \frac{1}{\sqrt{2\Pi}\ S}\ e^{-\frac{x^2}{2\ S^2}}$$

is the ideal limiting case to which all measurements of this sort approach; it is practically, however, never reached with any sort of perfection unless enormous numbers of individual measurements are made.

It is interesting to note incidentally that this normal curve is also the limiting value of the coefficients in an ordinary binomial expansion. If we expand the expression $(a+b)^n$ and plot the numerical values of the coefficients against the number of terms, the curve

(called the point binomial) approaches the probability curve as a limit as n is made indefinitely large.

When drawing the curve from a series of measurements (e.g., of the stature of a group of men) one first erects rectangles with the x axis as a base, the center vertical line of each rectangle being placed at the central stature of a small group whose stature differs from example by an inch. We thus get a series of rectangles erected on the x axis whose height represents the number of men whose stature is the same within one inch—e.g., from 68 to 69 inches, the center of this rectangle being set x = 68.5 inches; and so on for all the other statures. Such a plot is called a histogram. The upper center points of these rectangles are then joined by a smooth curve, which as has been said approaches the true probability curve as a limit as the number of individual measurements increases indefinitely.

Looking at the curve again from the standpoint of errors, it is seen to conform to the following specifications: the best representative value for the quantity being measured is the arithmetical mean of all the individual determinations; plus errors and minus errors occur with equal frequency; very large derivations from the mean occur very infrequently; small deviations occur more frequently.

It must be borne in mind also that the errors dealt with in this connection are truly random or accidental errors—the sort which remain in any measuring process after all known and correctible errors have been eliminated.

In the case where we are measuring some naturally occurring quantity which in itself varies to a much greater extent than the possible precision of the measuring process used, the deviations plotted on the curve represent primarily the natural variation of the quantity as it occurs in nature, and secondarily or not at all the precision of the measuring process itself. The difference may be illustrated by taking as an example of the first case an atomic weight determination; here the investigator is dealing with a quantity which actually has a definite univocal value which cannot be known by us as it is to the omniscience of God but which can be approached as a limit as the number and refinement of our measurements increase indefinitely.

The other case may be illustrateed by the conditions prevailing in plotting the stature of a large group of men; here the precision of the measurements is far and away greater than the actual variation of the quantity itself as it occurs in nature. We are not here concerned with plotting the errors of a measuring process but rather the natural variations of the stature of the men themselves.

Thus, Richards (5) performed 14 exceedingly accurate determinations of the atomic weight of lithium, all included within the limits of Li = 6.9382 to Li = 6.9409, average Li = 6.9396, the standard deviation being only 0.01% of the quantity being measured.

To illustrate the other cases, Scott measured with sufficient precision the blood sugars of 1000 rabbits and found them to vary between 74 and 168 mg. glucose per 100 cc. blood; the average of the 1000 rabbits was 124 mg., and the standard deviation was as much as 11.4% of the quantity being measured. To put the matter in a different way, Richards was trying to establish as closely as possible a representative figure for an actually existing physical quantity; Scott was trying to **make** a figure which would best represent a quantity which in itself does not exist—namely the blood sugar of the average rabbit. The average rabbit, like the average man, exists only in the Platonian ideology. The atomic weight of lithium, however, really does exist as a definiate univocal quantity to whose true value we can approach asymptotically.

We have seen that the values x and y in the probability curve represent respectively values of some given measurement (or deviation) and the frequency with which this measurement (or deviation) occurs. The most important feature of the curve, however, for our purposes is its integral—i.e., the area under the curve:

$$P = \frac{1}{\sqrt{2\Pi}\, S} \int_{x-i}^{x=j} e^{-\frac{x^2}{2 S^2}}$$

Where P represents the probability (as a decimal fraction of unity) that a given measurement will lie between the values x = i and x = j. If we take the entire integral between x = + ∞ and x = − ∞ we have P = 1, denoting the mathematical certainty that all the measurements lie within these limits; i.e., no individual measurement will have a greater value than + ∞ nor less than − ∞   . From the analytical properties of the curve we find that its second derivative is zero at x = S; and if the integral is taken between the limits x = + S and x = − S, the area between these limits is found to be P = .6816; i.e., 68.16% of all the measurements will be comprised within this range. If we integrate between the limits x = ± 0.6745S we find that P = 0.50; 25% of the measurements will be comprised in the region x = + .6745S and 25% will fall in the region x = − .6745S. In other words, the probability is that 50% of all the measurements will lie within the limits x = ± .6745S and 50% of them will be larger or smaller than this quantity. This particular deviation is called the "probable error":

P. E. = 0.6745S

The number of errors made in the process which are less than the probable error will be the same as the number which exceed it. As Mellor says, the probability is ½—that is the odds are even, or you may bet one pound against one pound—that the true value lies between the values: mean + P. E. and mean − P. E.

In modern statistical works the probable error is referred to only in passing; it is practically never used in practise. Fisher says, (p. 46) "The common use of the probable error is its only recommendation; when any critical test is required the deviation must be expressed in terms of the standard error in using the tables of normal deviates". Yet Bond on the other hand, the author of a recent (1935) treatise on probability primarily for physicists and chemists, uses the probable error as the basis for all his discussions and mentions the standard deviation itself only casually.

Owing to the importance and wide use of the standard deviation, the tables for the probability integral which are given in statistical works are usually set up in such a way that one reads off the probability (expressed as a decimal fraction of unity) not as a function of the actual deviate x but rather as a function of the ratio of the deviate to the standard deviation. That is, one enters the table at some value of $t = \dfrac{x}{S}$ and reads off the probability to which this corresponds. That is the way the table is set up in the 19th edition of the Handbook of Chemistry and Physics. In this table the value given for P is that for one half the curve only; the total integral at any given value of $\dfrac{x}{S}$ is found by doubling the corresponding tabular value Thus for example, to find what percentage of all the measurements may be expected to lie in the range x $= \pm$ S we take $t = \dfrac{x}{S} = 1$ and find the corresponding tabular value to be P $= 0.3413$; twice this value gives us the desired probability, or 68.16%. At x $= 0.6745S$, or t $= 0.6745$, P $= 0.25$; i.e., the probability is 50% that all measurements will be within the range x $\pm$ P.E., as was said above. As we go farther from the center of the curve the probability increases; thus for x $= 3S$ or t $= 3$ the half area is 0.4987 and the probability is 0.9974. This means that 99.74% of all the measurements should lie within the range $\pm$ 3S; in other words there are only 26 chances in 10,000 that a random error will exceed 3S — the odds against the occurrence of such a measurement are $\dfrac{9974}{26} = 384$ to 1.

Before going on to the next point I wish to emphasize two considerations which must always be kept clearly in mind when making any statistical studies: first, that the ordinary probability equation and the formulas derived for it are of little value, and even may be positively misleading when applied to a small series of measurements; and secondly, that no matter what system of analysis we use we are dealing with purely random accidental errors and not errors of method. If a man reads a burette wrong or performs an incorrect mul-

tiplication, or uses a grossly inaccurate method, about all the good a statistical analysis will do (in some cases) is to tell him what particular observation should be thrown out. If a chemist precipitates barium sulphate out of a cold solution containing e.g., chlorate of potash his reported mean value with perhaps an impressively small probable error is merely a scientific expression of self-deception, since he ought to know that his results are bound to be 8 to 10 parts per 1000 high anyway on account of co-precipitation. On the matter of the number of individual measurements, however, I shall have more to say presently.

What I have said so far about the standard deviation and the probable error has referred to values of these quantities as applied to individual observations; that is to say, these quantities show the grouping and variations of individual measurements about the arithmetical mean of them all. By them we can under certain conditions draw some conclusions regarding the relations of individual observations within a given series to the mean of the series. In many cases, however, both in analytical and physiological chemistry, it is of more importance to be able to find some relation between the mean values of two or more series of measurements. For example, I precipitate barium sulphate from pure sodium sulphate solutions in two series of experiments, all the experiments being conducted under identical conditions (as nearly as possible) with the single exception that in series A the solution is kept at the boiling point and stirred during the addition of the barium chloride while in series B it is kept at room temperature and is not stirred. Naturally the latter procedure gives the higher result. The question then arises, how certain can one be that this difference is to be ascribed to the variation in experimental procedure? There is, of course, a certain variation among the individual determinations in each series and a difference also between the mean values of the two series. How can one decide that this latter difference is not caused by the former?

The treatment of this question by means of the normal probability function requires merely the introduction of the term $\sqrt{N}$ in the denominator of the standard deviation:

$$S_m = \sqrt{\frac{\Sigma \, d^2}{N(N-1)}}$$

This quantity is often referred to as "the mean deviation of the mean"; Mellor (p. 525) calls it M, the mean error affecting the whole series of results; Scott in his article already referred to designates it by the Greek letter ε.

To answer the various questions already set with regard to analytical data (using this $BaSO_4$ recovery experiment as an example) we shall write down the necessary data. The standard deviation of the individual determinations and the standard deviation of the two means are calculated as already described.

	Procedure A	Procedure B
Arithmetical mean (% recovery)	99.63%	99.98%
S (for the individual determinations)	0.315%	0.559%
S (for the means)	0.088%	0.150%
N (No. of individual determinations)	13	14

I will put down the two questions that the analyst may have to solve and will then give the answer, using methods based on the normal probability function.

(1) Suppose that a given individual determination differs rather widely from the others, should it be retained or rejected?

There are two ways of going at this problem. First, we may take the deviation of the suspected observation from the mean of the series and see how much it exceeds the standard deviation of the individual determinations in the series. We then assign a priori some definite degree of mathematical probability to our proposed decision as to the fate of the suspected observation. Keeping in mind that very large deviations from the mean are almost certainly due to real errors and not to chance, let us say that we will reject any measurement whose deviation is so large that there are 997.4 chances in 1000 that it should be rejected. This being the probability we decide on, we enter the probability at P = 0.9974 (or P = 0.4982 if the table gives the area of half the curve) and see to what multiple of S this corresponds. This is found to be at $t = \dfrac{x}{S} = 3.00$; i.e., to that value of x which is three times the standard deviation. In the case under consideration there was one individual experiment in series A where the percent recovery was 100.29%, differing 0.66% from the mean of the series. Since S for the individual determinations in this series was 0.315% and 3S = 0.945%, we see that the questionable observation is within the range we have set and should, therefore, be retained.

Suppose now that we wished to be more meticulous; instead of being willing to accept a given measurement as really belonging to the series within the wide range of ± 3S, suppose we will throw it out if it differs only 2S from the mean. Then t = 2S = 0.630%; the actual deviation is 0.660%, and this particular measurement just fails of being acceptable.

The other system of handling this question is really the same method, but just turned around. One calculates directly the value for x for the suspected observation, looks up the corresponding value of $\dfrac{x}{S}$ of P, and from this decides whether or not to reject the determination. In this example the suspected measurement differs by 0.66%

from the mean of a series whose individual members show a standard deviation of S = 0.315%. Hence $t = \dfrac{x}{S} = \dfrac{0.66}{0.315} = 2.09$; the value for P corresponding to this 0.9634. This means that there are 3.66 chances in 100 that we are dealing with an accidental error, and 96.34 chances that a real error is present and that the deviation is significant. It is usually stated that a measurement differing from the mean by 2S (Fisher p. 45) (or 3 x P. E.) or more should be rejected since the odds are 21 to 1 against its validly belonging in the series. The value in the above example, therefore, is just about on the edge; under the circumstances, however, it would be safe to retain it. In a similar way w may decide about the most discrepant determination in Series B, which is 100.90, differing from the mean by 0.92%. The standard deviation for the determination in Series B. is 0.559% hence $\dfrac{x}{S} = \dfrac{92}{.559} = 1.65$, corresponding to P = 0.901; this observation should be retained. Some of the probabilities corresponding to different values of the deviate are given in the following table:

Value of $t = \dfrac{x}{S}$	Probability that the measurement does not belong to the series; error is a real error; difference is significant; measurement should be rejected.				Probability that the measurement belongs to series; error is accidental; difference is not significant; measurement should be retained.			
3.00	0.9974	997.4	chances	in 1000	.0026	2.6	chances	in 1000
2.00	0.9546	954.6	"	" "	.0454	45.5	"	" "
1.00	0.6826	682.6	"	" "	.3174	317.4	"	" "
0.6745	0.5000	500	"	" "	.5000	500	"	" "
0.25	0.1974	197.4	"	" "	.8026	802.6	"	" "
0.10	0.0796	79.6	"	" "	.9204	920.4	"	" "

In this matter of the rejection of suspected measurements it is evident that the lower we make the value of the "Coefficient of Certainty" $t = \dfrac{x}{S}$ the stricter we are with our criterion and the fewer measurements there are which we are prepared to admit to our series. Evidently too if the precision of measurement is much greater than the variation of the quantity being measured, or if the precision of measurement does not enter into the case at all then there can be no question of rejecting **any** measurement. Such cases would be, for instance, the measurement of a man's height or weight, the death rate of a city, or the numbers turning up on a group of dice. Here each individual figure is an open and shut case. A man just under 5 feet tall might be rejected as a candidate for the police force, but he could not be excluded from membership in a group of men just because his height differs by more than 3S from the average height of the group.

This matter of the rejection of suspected observations is treated rather differently by authors writing particularly for physicists and astronomers. They make use of Chauvenet's criterion, which the reader will find explained in Mellor (p. 563) and in Bond (p. 64) which, however, is not in vogue at all among modern statistical writers. The criterion comes to this: reject all observations which differ from the mean by more than V times the probable error, where V is a coefficient depending on the number of observations; its value for various sample sizes is given as

Number of Observations	V
10	2.9
20	3.3
30	3.5
50	3.8
100	4.2

For chemical practise where the number of observations is small we may take V = 3; and referring the criterion to the standard deviation instead of the probable error we find that this ctiterion would reject observations differing from the mean by more than (approximately) twice the standard deviation, which is also the norm given by Fisher (p. 45).

Going back now to one of the questions raised in analytical and biochemical work: (2) How certain can one be that the difference between the mean value of one series treated by procedure A and the mean value of another done according to procedure B can be ascribed to the difference in the experimental methods used? In the example given sodium sulphate solution gave a recovery of $BaSO_4$ 0.35% lower when precipitated hot with stirring than when the $BaCl_2$ was added at room temperature without stirring. Has this variation arisen by chance or by the difference in procedure? To answer this question we must deal not with the standard deviation of the individual measurements from their mean, but rather with the standard deviation of the two mean values themselves; i.e.,

$$S_m \text{ (Procedure A)} = 0.088\%$$
$$S_m \text{ (Procedure B)} = 0.150\%$$

Just as the standard deviation of the individual measurements indicates their dispersion from the mean of a very large number of separate determinations, so the standard deviation of the mean indicates the dispersion of one or more mean values from the final average of a very large number of mean determinations.

The usual procedure followed for the problem of this kind is to calculate first the square root of the sum of the squares of the standard deviations of the two means—a quantity often referred to as "the standard error of the difference":

$$s_d = \sqrt{S_{m1}^2 + S_{m2}^2}$$

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then calculate the ratio of $\dfrac{x}{S_d}$ (corresponding to the ratio t $\dfrac{x}{S}$ which we have just mentioned) and finally look up the probability corresponding to the calculated value of this ratio. In this case the two means differ by $99.98\% - 99.63\% = 0.35\%$, and the standard error of the difference is

$$S_d = \sqrt{0.088^2 + 0.150^2}$$
$$= 0.174\%$$

The ratio will be $\dfrac{x}{S_d} = \dfrac{0.35}{0.174} = 2.01$. The probability corresponding to this is now looked up in the table and comes out $P = 0.9556$. This indicates 955.6 chances in 1000 that the difference is significant; or in other words, there are only 44.4 chances in 1000 that this difference could occur as an accidental error; or that the odds are 21.5 to 1 that the excess $BaSO_4$ must be ascribed to the addition of the $BaCl_2$ to the unstirred solution at room temperature.

As another example we may take a case given by Shewhart. Samples of cable were submitted by two companies and on testing gave the following results:

	Company A	Company B
Tensile strength (mean)	13550	13978
(lbs. per sq. inch)		
No. of samples tested	20	20
S (for individual cables)	358.6	431.8
S (for the mean)	80.19	86.05

The question is, is there a real superiority of the cable of Company B, or is the difference between the 2 sets due perhaps to the random variations among the samples? Again we calculate the standard error of the difference:

$$S_d = \sqrt{(80.19)^2 + (86.95)^2}$$
$$= 117.6$$

Then calculate the ratio of this to the difference in tensile strength between the two sets of cables:

$$\frac{\Delta x}{S_d} = \frac{428}{117.6} = 3.64$$

Finally look up the probability corresponding to this, which in this case is to all intents and purposes unity: $P = 0.9998$. That is to say, the cables of Company B are unquestionably superior to those of Company A, although probably this superiority is not of much consequence since one set is only about 3% stronger than the other. The reason why the statistical certainty is so high is the fact that the standard errors are quite small, amounting only to 3% of the quantity being measured.

(To be continued)

EXEMPLARY USE OF CHEMICAL ABSTRACTS

Bernard A. Fiekers, S.J.

A genuine "Vade Mecum" in laboratory and library finds its basis in that special publication of Chemical Abstracts: the well known "List of Periodicals", sometimes called the "Library Index". One copy of the Abstracts went through the evolution of being at first interleaved, and finally, to enlarge its capacity, it was taken from its paper binding, slit, punched and put into a small loose leaf binder.

Gradually its factual content was enlarged. For certain journals, the call numbers of city libraries comprised the first handy annotation. A few notes on old and new serial numbers were then incorporated. It accompanied the writer on every visit to a library at the time, and got him out of the same well before closing time. There was no time wasted when certain libraries listed the volume and division numbers according to a series different from those given in the ordinary footnote reference.

The book saw him through a two semester hour course in Bibliography. Here the evolution of new journals out of old ones, the branching of the journals in the years as the result of intensified specialization, the origin of new journals, and the ideas that attended their birth, the outstanding editors and contributors,—all of these were discussed and merited annotation according to their practical importance.

In the laboratory, the list became ever handier. Short reference notes on the technique of using Reilstein, Landolt-Bornstein and the International Critical Tables were added or expanded. Bibliographies of subjects sometimes found a place. In one case, the prospectus from the publisher was found valuable for the purpose. It was a list of the Chemical Monographs. One could see at a glance where the Chemical Rubber Company offered any complete treatises bearing on the particular problem at hand.

At the desk, the book proved itself equally useful. Sometimes when the next day's work had to be outlined, certain problems of apparatus, purification, etc., could be forecasted. In one of these problems of purification, conflicting methods were reported in the literature. To come to a decision, one had to have recourse to the "Organic Syntheses Series", because all methods therein given are well checked. A method for purifying a homologue was found and good results were assured. The contents of Organic Syntheses were required in a form so that one could see them at a glance. The pages of Wiley's catalogue describing this set filled the gap and promptly found a place in the work under discussion.

The solution of apparatus problems found an excellent start when

the indices of "Baker's Chemist-Analyst", "Synthetic Organic Chemicals" and those of THIS BULLETIN—in all, a matter of a few pages—were inserted into the collection.

The thing was shaping itself not only for research, but as well for a course in the chemical literature that might be given on this basis. One problem was that of predicting the boiling point of a compound, the vapor pressure data of whose homologues ran along a whole gamut of reduced pressures. Suitable apparatus had to be selected, once the approximate range was known, and there was not much time to be spent on the problem since it played a relatively minor role. Such data was selected from the trade catalogues, like Eastman's. It was found that in some cases, Eastman had a different set of temperature pressure relations in its successive annual publications. It was close enough for an efficient selection of apparatus and a satisfactory determination was made. Of course the experience was annotated for possible later use.

Patterson's: the "Literature of Chemistry" (Wiley, N. Y.), and Reid's "Introduction to Organic Research" were often consulted and, where practical, annotated. Annotations grew and the weeding process kept apace. If the writer were active in this work today, he would most surely include what promises to open a new field in the literature of science. It is the "Bibliofilm and Auxiliary Publication Services" of Science Service. Principles for the criticism of articles published through the latter are detailed in Document No. 173. This document forms part of the literature sent free on request by Science Service, 2101 Constitution Ave., Washington, D. C.

On the whole, this "Vade Mecum" demanded a little armchair work from the chemist. But in the long run it went its way in bringing both the arm chair and bench work to a rational ratio. The compiler of this "scrap-book" was not the only recipient of its benefits. Many, from students to the college librarian, have thumbed its pages. It has served a bibliography course, general laboratory and library references, and some research. Will it form a good basis for bibliography lectures? That has to be tried.

LABORATORY SUGGESTIONS

CARBON RESIDUES. A very efficient way of removing carbon residues from distilling flasks is to soak the residue for a few minutes in solution made with two tablespoons of trisodium phosphate and one tablespoon of sodium oleate in a liter of soft water. Chemical Analysis, Jan., 1934.

PURE ANHYDROUS SODIUM CARBONATE. "Washing the so called c.p. anhydrous carbonate with ethyl alcohol and drying at 110 deg. render it satisfactory as a volumetric standard. Anhydrous sodium carbonate does not decompose below 450 deg., nor does it change its crystal structure below 375 deg. The impurities leading to high results are very probably due to sodium oxide and (or) sodium hydroxide which come from the decomposition of sodium bicarbonate in its conversion to sodium carbonate." J.I.E.C., Anal. Ed. Sept., 1934.

PHYSICS

WAVELETS OR PARTICLES?
(Abstract)
JOHN P. DELANEY, S.J.

It is too little known that Huygens was not the first to suggest the wave theory of light. Father Francesco Maria Grimaldi, S.J., was the first to suggest that light consisted of waves, a suggestion clearly based on his brilliant discovery of the phenomenon of diffraction.

Father Grimaldi was a deeply learned man, an expert in experimental physics, who chose optics as his principal field of research and penetrated more deeply into his subject than any of his predecessors. His observations were published in a voluminous treatise, Physico-Mathesis de Lumine, Coloribus et Iride, 1665, a publication ante-dating Huygens work by a quarter of a century.

It is interesting to note that the process of diffraction, discovered and so named by Father Grimaldi, and cited for nearly three centuries as proof of the wave theory of light, has been applied recently to prove that matter itself is wave-like in nature. Remarkable diffraction patterns have been obtained with both electrons and protons, the fundamental units of all matter.

SEISMOLOGY

NOTE

Four Jesuit members were present at the Sixth International Union of Geodesy and Geophysics held in Edinburgh, England, during August. Those in attendance were: Father Macelwane from St. Louis, Father LeJay from Zi-ka-wei, Shanghai, China, Father Rowland from Stonyhurst, England, and Father Lynch from Fordham University, New York. Papers on Seismology were submitted by Fathers Macelwane, Delaney, Repetti and Lynch. These papers will be published in the Proceedings of the Union.

News From Spain

In the public press on September 19th, it was reported that Father Vitoria, S.J., the world renowned chemist and holder of the Nobel Prize, was shot and killed in the Spanish Revolution. Father Vitoria was an octogenarian, but neither his great age, his great work, his universal prestige nor his admirable intellectual and personal worth halted his executioners. He was merely a despised clerical, a Jesuit.

It is to the genius and labor of Father Vitoria that Spain owes much of her progress in recent years. He gave to his country a group of chemical engineers who could develop native resources and supply Spain with much that formerly she had to obtain abroad. His reputation as a teacher was such that his students were received without question by foreign universities. His works have been translated into all leading languages and serve as authoritative reference works in Europe and America.

On October 17th, we received word from one of the Spanish Jesuits at Valkenburg, Holland, that Father Vitoria was *not* dead. The fact is, that Father Vitoria and two other priests faced a firing squad. Father Vitoria's companions were instantly killed, but he was only wounded; he feigned death and at the opportune time, he escaped. He flew from Spain and is now at Bellengo, Italy. Father Vitoria is a witness to the fact, that he heard the Communist Officer say: We shoot you because you are priests. This testimony is most important for future investigations.

This Spanish Jesuit is certainly a famous scientist, and we do not wish to take away from his glory,—but the truth is that he did not receive the Nobel Prize. In a future edition of the BULLETIN we hope to print a list of the publications of Father Vitoria.

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Canisius College, Biology Department

The department of Biology has recently acquired several new sets of models in color showing the development of Amphioxus and the Frog; also cross sectional models showing the development of the mesoderm in Amphioxus; then too, another set of models that show

by skeletal preparations the evolution of the chondocranium, skull, vertebrae, pelvic and pectoral girdles in Vertebrates.

A new, one year course, in Genetics is offered as an elective to seniors in the Bachelor of Science course. Ten students are enrolled, and Dr. Lorz is the Professor in charge. Dr. Elliott, Professor of Histology, is giving a course in: Medical Essentials for Social Workers, to the students in the Social Welfare School.

The Mendel Club has arranged a splendid lecture course for the present year. The lectures are given in the evening. Weekly field trips are made by the members of the Mendel Club, for the purpose of collecting valuable zoological and botanical specimens. The Club is preparing a number of biological exhibits and demonstrations, which will be displayed in the college library.

The lists of journals found in the library of the biology department are: Biological Abstracts, Botanical Review, Journal of Heredity, the American Naturalist, Science, Scientific Monthly, Science News Letter, Quarterly Review of Biology, Questions Scientifiques, Physiological Zoology, Practical Microscopy, Nature (London), Journal of Cellular and Comparative Physiology, Journal of Experimental Zoology, Journal of Comparative Neurology, Journal of Morphology, American Journal of Anatomy, The Anatomical Record and Biological Bulletin.

Loyola College, Baltimore, Maryland, Chemistry Department

At the annual meeting of the American Chemical Society held at Pittsburgh, Pa., from September 7th to 12, Rev. Richard B. Schmitt read a paper before the Micro-chemical Section. The title of the paper "Micro-vaporimetric Determination of Molecular Weights with Macro-analytical Balance." The lecture was illustrated with lantern slides and motion pictures.

The Loyola Chemists' Club is now in its eighth year. The program for the present scholastic year is completed and so far two lectures were presented.

On Thursday, October 29th, Dr. Walter A. Patrick, of Johns Hopkins University, lectured on the subject: "Ultramicroscopic Notes on the Colloidal Particle."

Dr. Beverly L. Clark of the Bell Telephone Research Laboratories, New York City, delivered an interesting lecture to the Loyola Chemists' Club, on Thursday, November 12th. His subject was: "Applications of Micro-Analysis in Telephone Research." The lecture was enthusiastically received by the large audience.

Dr. William M. Thornton, Jr., formerly at Johns Hopkins University, is on the Chemistry Faculty; his position is: Research Fellowship in Chemistry.

Papers given at various scientific meetings:

W. C. Hess and M. X. Sullivan. The cystine content of deaminized proteins. The Federated Societies for Experimental Biology. March 25-28, 1936. Washington, D. C.

M. X. Sullivan. Further studies in muscular dystrophies. Ibidem.

W. C. Hess and M. X. Sullivan. Reaction of cystinylglicine and other cystine peptides in the Sullivan cysteine reaction. American Chemical Society. April 13-17, 1936. Kansas City.

M. X. Sullivan and W. C. Hess. Reaction of methylguanidine with 1.2—naphthoquinone-4-sodium sulfonate. Ibidem.

M. X. Sullivan and W. C. Hess. Estimation of cystine in urine. Ibidem.

M. X. Sullivan, W. C. Hess, and B. L. Brock. The cystine content of the finger nails in advanced tuberculosis. Ibidem.

The papers published during the year 1935-1936:

W. C. Hess and M. X. Sullivan. Separation of guanidine and methylguanidine by means of beta-naphthalenesulfonyl chloride. J. Amer. Chem. Soc. 57, 2331, 1935.

M. X. Sullivan. A colorimetric test for guanidine. Proc. Soc. Exp. Biol. Med. 33, 106-8 (1935).

M. X. Sullivan and W. C. Hess. Structure of colored compound formed in the Sullivan reaction for guanidine. Proc. Soc. Exp. Biol. Med. 33, 220-2 (1935).

M. X. Sullivan and W. C. Hess. The colored compound formed in the Sullivan reaction for guanidine. J. Am. Chem. Soc. 58, 47 (1936.)

M. X. Sullivan, W. C. Hess and Filadelfo Irreverre. Studies in muscular dystrophies: The presence of simple guanidine derivatives in the urine. J. Biol. Chem. 114, No. 3, July 1936.

The following papers are in press:

M. X. Sullivan and W. C. Hess. The determination of cystine in urine. J. Biol. Chem...............

M. X. Sullivan. An examination of the Sullivan colorimetric test for guanidine. J. Biol. Chem...............

Boston College, Physics Department

Due to the numbers of students in the post-graduate department, new laboratories have been constructed. Part of the equipment for research in electronics includes: Decade-resistance boxes, precision condensers, standard inductance bridge, oscillators, amplifiers and Dumont electron oscillograph. Professor Gager, formerly with the faculty of M. I. T., directs the research work.

Dr. White directs the research work in Advanced Mechanics and Theoretical Physics. The laboratory for experiments in light is now equipped with a Gaertner interferometer and a new spectrograph.

In the Physics Library, the following journals are found for the research students: Physical Review, Review of Modern Physics, American Physics Teacher, Physics, Review of Scientific Instruments, Physics, Philosophy of Science, Scientific Monthly, Science Abstracts (Section A), Electronics, R. C. A. Review, Q. S. T. and Radio.

Holy Cross College, Physics Department

According to the new schedule, in which Physics is optional to the members of the Junior class, ninety-three students elected Physics. This course consists of two lectures and one laboratory period each week. The text-book is: "An Introductory Course in College Physics," by Black.

In the Pre-Medical course, there are three lectures and one laboratory period each week. The text: "College Physics", by Foley. The Bachelor of Science students in Freshman and Sophomore use as their text: "Physics," by Hausman and Slack.

Fordham University, Department of Biology

A Promar Microscopic Drawing and Projection Apparatus has been added to the equipment of the Biology Department of Fordham University. This instrument can be used for projection, a drawing lamp, a microscope and as microphotographic apparatus. It is equipped with a micro-cuvette for projection of free swimming organisms. A special insect holder allows projection of a complete insect or parts of insects.

A special Botany and Bacteriology laboratory is being constructed. This laboratory will have the most modern equipment such as sterilizers, incubators, etc. The tables for this laboratory were specially designed by the staff for Fordham.

A number of private cubicles for research students have been constructed. Each of these compartments is in reality a small laboratory. These structures will fill a long desired need and will add greatly to the morale of biological research.

A new course has been added to the graduate division of Botany. This course is titled "Problems in Flower Behavior" and has drawn a number of graduate students who are advantageously using the greenhouse for the laboratory work in this course.

Among the recent additions to the library is a remarkable set of fourteen volumes by Hough, "The American Woods." This work is augmented by a section of every wood that grows in United States also the ten volumes of Handworterbuch der Naturwissenschaften.

Manila Observatory, Manila, P. I.

Father Jose Coronas y Voera celebrated his Golden Jubilee in the Society. Father Coronas was born in Barcelona, Spain, on January 8, 1871. He entered the Society of Jesus on September 30, 1886. In 1894, he was sent to Manila and was attached to the meteorological department of the Observatory from 1894 to 1901. He was ordained to the priesthood in St. Louis, Mo., in 1904. After three years, he returned to Manila and was head of the meteorological department until 1931. He is now attached to the faculty of the Ateneo de Manila. The BULLETIN takes this occasion to congratulate the Jubilarian on his long service at the Manila Observatory.

Father Charles E. Deppermann delivered a lecture to the officers of the U. S. Army at Fort Santiago, on Wednesday, September 2, 1936. The subject: Tropical Storms and Climatic Conditions in the Philippines.

On Sunday, September 6, W. H. Clover, of Pan American Airways was host at a dinner at the Manila Hotel, honoring the officials of the Manila Observatory, Major E. H. Bowie, who is in charge of the weather bureau of San Francisco; and Lieut. A. E. True, of the U. S. Navy Department at Pearl Harbor, Honolulu. The guests included: Father Michael Selga, Father Charles E. Deppermann, Father William C. Repetti, Father Bernard E. Doucette, Father Edmund J. Nuttall, Major Bowie, Lieut. True, Lieut. C. A. Chappell, U. S. N., Lieut. Barzen, U. S. N., and Gordon Mills.

Department of Chemistry

The chemistry department of the Ateneo de Manila announced a series of new courses in Industrial Chemistry. The courses were planned to give to the students a thorough working knowledge of the fundamentals and theories, as well as actual applications of many industrial problems. The Faculty comprises a staff of five Chemists to further this new endeavor. These new courses include: General Industrial Chemistry, Leather Manufacture, Industrial Fermentation, Soap and Cosmetic Manufacture, Industrial Mycology, etc. The complete four years' course, including the regular courses of undergraduate work, lead to the degree of: Bachelor of Science in Industrial Chemistry. Rev. Eugene Gisel, S.J., is the Dean of Chemistry and Rev. Henry B. McCullough, S.J., the Dean of the College.

John Carroll University, Cleveland, Ohio

Within the year, John Carroll University will celebrate its Golden Jubilee, and appropriately at its new location in the exclusive suburban section of Cleveland. The group of buildings is composed of:

Administration Building, Students' Residence Hall, Faculty Residence, Chemistry Building, Physics-Biology Building, Auditorium and Power House.

The Biology Building contains four laboratories, lecture-room, dark-room, preparation-room, library, professor's office, professor's research laboratory, cold-room and technician's laboratory.

The Physics Department occupies the ground floor of the Biology Building. There are three large laboratories, lecture-room, dark-room, motor-generator room, professor's office, technician's laboratory, and two store-rooms. The electrical equipment all through the department is the latest design.

The Chemistry Building consists of three floors and a basement with a total floor area of 30,000 square feet. There are lecture-rooms, several laboratories, library, research laboratories, balance room, director's office, private laboratories, stock rooms and combustion room.

Weston College

Seismological Observatory. The extensive additions to the observatory under construction during the past few months, which are to contain the new Benioff seismographs and auxiliary apparatus, have been completed. The Benioff vertical component has been in operation since July 6th. Since that time eighty disturbances of distant and local origin have been recorded. The other two components will be in operation about December 1st.

Our Observatory cooperated with Harvard University in the recording and timing of several blasts in the New England region. Data from these blasts will be used to construct travel time tables for this region. We have been using a National FB-7 receiver for these experiments, and the service of the Ottawa continuous time signals.

Recent equipment in the laboratory includes a Pako photo-dryer, which is used for drying the records. The drying time has been reduced from several hours to three minutes. An air-cooled Balopticon provides us with a means of projecting the short period records on graph paper. This facilitates accurate and easy reading to 1/200 of a minute. The Bosch Omori and Wiechert instruments are operating as usual.

Recent visitors to the Observatory were Doctors Slichter, of M. I. T., Perry of Williams College, Leet and Taylor of Harvard University, Father Macelwane of St. Louis University and Dr. Snow of the University of Colorado and Harvard.

Father G. A. O'Donnell is Director of the Observatory, and Father D. Linehan, the Assistant.

Biology Department.—The courses in Biology cover a period of three years. These courses include the following subjects: Funda-

mental Biology, the Nervous System of Man (as applied to Experimental Psychology), General Zoology, Fundamental Botany, Comparative Anatomy, General Physiology, Fundamental Embryology and Genetics.

Chemistry Department.—Analytical Chemistry (Qualitative Analysis) is now taught according to the Semi-Micro method of Dr. Carl Engelder. Cf. Article in this issue.

Physics Department.—There are four Scholastics engaged in advanced courses in Physics.

Geology and Anthropology.—There are more than three thousand mineral and fossil specimens available for those interested in the sciences of geology and anthropology. Then too, there are about two thousand lantern-slides for use in geology, and several motion-picture films. There are on file more than three hundred geological maps. The lecture-room is equipped with projection apparatus for all purposes. There are two Scholastics from Weston College now engaged in graduate work in anthropology: one is at the Catholic University, Washington, D. C., and the other at Cambridge University, England.

Lectures:

On Friday, October 9th, Father M. J. Ahern presented a lecture to the Northeastern Section of the American Chemical Society, entitled "A Chemist Looks at Earthquakes". Slides were shown of geologic faults, mechanics of earth disturbances and the various types of instruments. These were supplemented by a motion picture "Earthquakes", which was arranged by the Harvard University Dept. of Geology.

On November 1st Father G. A. O'Donnell lectured to the Catholic Alumni Sodality on "Earthquakes and Earthquake Detection." As a great part of the audience were men engaged in science, an interesting discussion followed.